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THE PUBLIC HEALTH WORKER AS AN AGENT OF SOCIO-CULTURAL CHANGE

Cora Du Bois

OPERATIONAL RESEARCH IN HEALTH EDUCATION

Kenneth D. Benne

SOCIETY OF PUBLIC HEALTH EDUCATORS

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THE PUBLIC HEALTH WORKER AS AN AGENT OF SOCIO-CULTURAL CHANGE

CORA DU BOIS*

This is the Third Annual Dorothy B. Nyswander Lecture. It was delivered in Berkeley, April 24, 1959. The lectureship was established in honor of Dr. Nyswander upon her retirement as Professor of Health Education at the University of California.

I need not underscore my pleasure at being asked to join you in honoring Dorothy Nyswander. Most of us here this evening have honored her for many years, but it is pleasant to have annually a public and formal opportunity that serves to punctuate our continuing esteem. But the very purpose of this occasion places serious responsibility on a speaker. The title selected nonchalantly last fall is a further challenge. Fortunately, the intervening months have been spent at the Center for Advanced Study in the Behavioral Sciences where many ideas are in ferment and there are many intellectual resources. To those intervening months and the Fellows at the Center, I am deeply indebted.†

Both analysts and practitioners in the contemporary world have an almost compulsive interest in change — and small wonder! The Western World has undergone in the first half of the Twentieth Century, changes so momentous and so often stressed that they need no comment. Similarly, at least the leadership of the non-Western World holds aspirations for their societies that promise changes there even more dramatic than those experienced in the West.

It is not so much what these changes are, in a descriptive sense, that need concern us here, as the question of understanding how they occur. More particularly, as public health workers you are concerned with change that is both induced and planned. Neither revolutionary processes of change nor accidental ones (if there be any) need preoccupy us tonight.

The Intrapsychic Approach to Change

The approaches to change are as varied as the discipline concerned with the question. Psychologists are likely to see change from inside the individual looking out. They perceive change as alterations in people's perceptions, cognitions, affects, and attitudes, and on through the armamentarium of concepts used by psychologists. For example, last year Dr. Gordon Allport of Harvard, who delivered the second Dorothy B. Nyswander Lecture, spoke of "Perception and Public Health,"(1) and

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† I am also greatly indebted to Dr. Melford E. Spiro for a careful reading of this paper. Dr. Jerome Frank was also kind enough to give me the benefit of his advice. It is a pleasure to acknowledge my debt to both.

gave wise and learned counsel on personal and cultural proceptual differences, and the role they may play in public health work. Or, for example, Dr. Leon Festinger in his recent book on "A Theory of Cognitive Dissonance"(2) gives us a most important lead into the dynamics of the strain for consistency and its bearing on attitude changes. He points out that cognitive dissonance arises when new events, new information, or decisions face an individual. Through the process of rationalization the individual attempts to reestablish consonance, i.e., consistency, by changes of cognition, or changes of behavior, or changes of values. However, if the discrepancy between established and new information is too great, one way of maintaining consonance is to reject the new. While it is important for the public health worker to realize that the strain for consistency may work for him in inducing change, it is equally important to realize that it may work against him, if the gap between the old and the new is too disturbing. People will avoid individuals and situations that are likely to increase dissonance to the point of discomfort. In sum we can say: The higher the involvement the greater the dissonance, and the more vigorous the rejection of the new. Since cognition is primarily determined by reality (or at least reality as it is proceived – to use Dr. Allport's term) relevance becomes an important issue. If two facts are opposites in a logical sense, that is, if two facts cannot be put into one category; if they are mutually exclusive and exhaustive, that means they are likely to arouse dissonance on a cognitive level. But if two facts are simply irrelevant, if they appear to bear no relation to each other, then cognitive dissonance is presumably in abeyance. For example, a thing cannot be both red and green. To insist that they are is to arouse dissonance. But to know that roses are red and sugar is sweet is to know two irrelevant facts.

In arguments of this kind examples are often helpful. For example, if a health educator enters a community where it is commonly held that fever is caused by a grandfather's punitive ghost and if the health worker proclaims that this belief is false and that the truth is that fever is caused by mosquitoes, he will precipitate dissonance. But it will be a dissonance difficult to test. The health worker will be hard put to prove that grandfather's ghost does *not* cause fever and equally hard pressed within the proceptive framework of the community to prove that mosquitoes *do* cause fever. On the other hand, if the health worker merely says that mosquitoes breed in water and produce fever, he will be engaging in irrelevances within the proceptive framework of that community. In the first instance, the health worker may be avoided as a person who precipitates unpleasant dissonances. In the second instance, even though the health worker may succeed in having all the standing water drained, he has made no headway precisely because no cognitive dissonance has been aroused. When the health worker leaves, the ameliorative measures will

be abandoned. No more than irrelevant activity has taken place. The resources for change inherent in cognitive dissonance have not been tapped.

But now let us suppose that a health worker claims that injections cure yaws whereas the common belief in the society is that poultices cure yaws. Here is dissonance, but dissonance subject to the reality testing of cognitive powers. The strain for consistency will be enlisted to change beliefs and behaviors. Injections and the cure of yaws can be cognitively brought into consonance. A *technical* change has been successfully introduced. When preventive and curative medicine are isolated from each other, the possibility of reality testing in the technical dimension of culture is diminished. In my opinion, this separation of preventive from curative medicine was an important element in reducing the success of the Rockefeller health demonstration units in India during the 1920 and 1930's. Speaking theoretically, exclusive emphasis on preventive medicine minimizes the possibility of reality testing and fails to exploit cognitive dissonance and the strain for consistency in the human personality.

We must return to this subject later in a different context — particularly in the context of designating the change described as a technical one.

The Interpersonal Approach to Change

Although many other intrapsychic sources for change could undoubtedly be found, my intention is not to survey systematically but only to illustrate. So, let us shift from the intrapsychic to the interpersonal level in the appraisal of change. Osgood and Tannenbaum, in an article discussing congruity and attitude changes, conclude that if a highly regarded person holds views different from one's own, the change may be either toward a lower regard for the person or a modification of one's opinion. The higher the esteem in which the person is held, the more likely it is that the opinion will change.(3) Merton reaches analogous conclusions. Speaking of value-homophily between friends he says, "In the give-and-take of the friendship, initial divergences of value tend to be reduced. If the friends have an approximately equal emotional stake in the relationship, this is likely to occur through mutual accommodation of their values. If one is more deeply involved in the relationship than the other, his values are more likely to be modified to accord with the values of the less deeply involved."(4)

Kelman(5) working at a comparable level of analysis adds further dimensions to Merton's and Osgood's proposals. Kelman charts the antecedents and consequences of three different processes of attitude change which he labels compliance, identification, and internalization. It is difficult to do justice to Dr. Kelman's argument in so brief a condensation.

However, let us select some diagnostic features of these three processes and assume for our present purposes that the agent is the public health worker and that the actor is the person he hopes to persuade.

In compliance, the agent of change possesses means of control or surveillance. The actor adopts the induced behavior because he expects to gain specific rewards or approval and avoid specific punishment or disapproval by conforming. The response of the actor, however, does not become part of his framework of values.

Any health worker who has done field work will readily recognize this type of change, particularly if he has worked in underdeveloped countries where he has entered a community, or served in a newly established Ministry of Public Health, backed by the prestige of a powerful government or of an international agency. Where change depends on compliance, permanence of change is unlikely. As soon as the foreign worker leaves, the innovations that were initiated are likely to be abandoned.

Now let us turn to the process of attitude change called identification by Kelman. In identification the agent of change is an attractive figure. The actor accepts influence because he wants to establish or maintain a satisfying, self-defining relationship to the agent. This is congruent with what Merton has to say about value-homophily in friendship. The actor believes in the responses which he adopts, but their specific content is more or less irrelevant. The new response is accepted as a value, but it is isolated from other values held by the actor.

Again any experienced health worker can recall example after example of this kind of experience. One that comes to mind is of a Minister of Health, recently returned from training abroad, who called out the army to administer mass inoculations in the capital city on market day, when people streamed in from the countryside. In this case, the Minister of Health had identified with Western medicine and wanted to define himself as related to it. The people inoculated obviously only complied.

Kelman's third process of attitude change, internalization, represents the kind of change all serious health workers wish to achieve. In internalization, the agent of change is credible rather than just attractive. The actor's cognitive field is reorganized. (To use Festinger's language the force of cognitive dissonance is utilized to establish consistency.) The actor sees the nature and utility of the new behavior and perceives its relevance to issues. The new is not only valued; it is also integrated with other values.

I should like to suggest that Kelman's three processes of attitude change, seen from the viewpoint of an anthropologist studying acculturation, may have diachronic (i.e. sequential) implications. This also is a point to which I shall return later.

In any event, Osgood, Merton, and Kelman, all see change as a function not only of intrapsychic forces but also of interpersonal relations. The implications for the public health worker are legion. The credibility and/or the affection in which he is held in any community, the kinds of interpersonal relations he is capable of establishing, are crucial factors in effecting change. But equally crucial are the persons in the community who are prepared to establish a relationship with the health worker. Induced or planned change can be viewed as a relationship between agent and actor. Readiness must exist in both. Such formulations remove from the field worker's shoulders exclusive responsibility for the success of a program. They should place in more realistic context the goals that planners may legitimately formulate for practitioners.

The importance of such interpersonal relationships are stressed because they have been insufficiently emphasized by technically minded planners. Personal relationships and the sociology of small groups make up a relatively new field of investigation in the social discipline. Important as that field is, it remains still largely unassimilated into other less interstitial fields of social inquiry.

But please understand me, while stressing the importance for change, of interpersonal and small group relations, I am not claiming that all elements of change reside in such interactions, but only that it is a significant area and one too often ignored by those who plan and carry out health programs.

No one would claim, for example, that the introduction of cattle, disease, and western artifacts into the Hawaiian Islands toward the end of the 18th century and the beginning of the 19th were insignificant in producing changes. But it could be argued that the long and faithful service rendered by a British seaman to King Kamehameha I was equally formative. Also it could be argued that the early missionaries were potent influences leading, after the death of Kamehameha I, to the repudiation by his queen of the whole tabu system on which Hawaiian social relations and subsistence rested.

In the same vein, the defeat of Japan in World War II and the policy of democratization pursued by SCAP (Supreme Commander for the Allied Powers) were certainly not negligible. But the results were not always in the direction foreseen. Nor had they always the hoped-for permanence. What was perhaps *not* foreseen was the very great influence of the G.I. in his interpersonal relations and particularly in contracting legal or illegal relationships with Japanese women. In fact, so little was the potential for change foreseen in these informal personal ties that every effort was made to restrict them.

In any society that tends to view life technically, to persuade by mass media, to fragment people into congeries of traits appropriate for statistical analysis, to see change in terms of goals that must be

achieved in one to five years – in any such society it becomes necessary to plead the importance of interpersonal relationships and of small groups if this significant dynamic for change is not to be overlooked. Trusted counselors and friends, wives and mistresses, parents and children, esteemed colleagues, can often make or break the best laid programs of planners.

Modes of Adaptive Behavior

Let us now turn from these intimate, though dynamic, forces bearing on socio-cultural change to another work of Robert Merton. In *Social Theory and Social Structure*(6) Merton discusses deviant behavior. He suggests five categories of role behavior that describe *modes of adaptation*, not personalities. The five proposed modes of adaptive behavior are: conformity, innovation, ritualism, retreatism, and rebellion. Although Merton discusses these categories in terms of role behavior within the American cultural scene, I believe they may have cross-cultural implications, since Vogt, working on acculturation processes among Navaho veterans, independently emerged with some analogous categories.(7) I also believe that Merton's categories of adaptation may provide the public health worker with at least some preliminary guides for identifying groups or individuals who may be crucial allies, or deadly opponents, in effecting social changes.

Conformity, as a mode of adaptation, means that cultural goals are closely meshed with institutional means for their achievement. Groups or individuals possessing this mode of adaptation are not likely to be easily changed. They represent the stable and conservative elements in a society although this does not necessarily mean that they are its most powerful or successful members. The French peasantry of the early Twentieth Century or the upper middle class of small urban communities in the United States might possibly be considered examples of groups for whom goals and the available institutions mesh. There is no gap or at least a minimum gap between aspirations on the one hand and means for their realization on the other hand. If their goals do not already coincide with those of the public health worker, they will not be easily persuaded to change. They would at most react in the manner proposed by Kelman in his compliance category. If on the other hand, their goals do coincide with those of the health worker, Merton's conformist mode of adaptation would coincide with Kelman's proposal internalization category.

Innovation, as a mode of adaptation, means by Merton's definition that there are no, or inadequate, institutional means for the achievement of cultural goals. In other words, there is a gap between aspirations and ways of implementing them. Groups or individuals in this position are probably the most receptive to institutional innovations. Psychologically they may be considered to fit Kelman's identification category. I suspect

that the leaders of many of the new nations belong to this mode of adaptation They have accepted Western goals and wish to create the institutions necessary to support them. With such groups the role of the public health worker is relatively, but only relatively, easy. The task is the creation of cadres of trained personnel and the institutional frame work within which they can work. The task, with such groups, is not to persuade, nor even to achieve the goal of improved health, but rather to provide such innovators with ways and means for them to build their own appropriate, supporting institutions for goals the health worker and innovators share. I see this task as the primary and most feasible one facing the World Health Organization or our own overseas health assistance program. Since these agencies can operate in countries only on the invitation of governments, they are assured of a group already sharing somewhat the goals of Western medicine. The international or foreign agency's task is then to assist such groups in establishing effectively organized and staffed departments of public health.

Merton's third mode of adaptation is ritualism. In ritualistic groups or individuals the cultural goals are muted compared to a compulsive insistence on institutional means. Groups and individuals to whom this mode of adaptation is congenial are not hard to identify. Bureaucracies and professions are frequently, and the world over, prone to ritualistic adaptation. Here the role of the public health worker is fraught with a double danger. Our own medical and health traditions frequently encourage ritualistic behavior at the expense of ultimate goals. Vaihinger's law of "the preponderance of the means over the end" has in my observation often led medical and health workers to insist on forms at the expense of content. Ritualism as a mode of adaptation is a professional hazard. When it runs head on against another group's ritualism, deadlock, rather than change, too often results.

The fourth mode of adaptation suggested by Merton is retreatism. Here both goals and institutional means are rejected. As Merton says, "people who adapt (or mal-adapt) in this fashion are, strictly speaking, *in* the society but not *of* it."(8) Like those who resort to ritualism, retreatists are not hard to identify. They may be objects of concern for the public health worker but they are not fulcrums for induced socio-cultural change. We need not linger over this mode of adaptation.

Merton's last and fifth mode of adaptation is rebellion. He states, "When the institutional system is regarded as the barrier to the satisfaction of legitimized goals, the stage is set for rebellion as an adaptive response.(9) There is no doubt that the rebellious mode of adaptation can be the source of massive socio-cultural change. It is also true that in certain communities the public health worker may inadvertently foster the rebellious adaptation or be a focus for those individuals and groups who find such an adaptation congenial. Nevertheless, since most health

workers do, and must, work within institutional boundaries, to use these potentially powerful groups and individuals to effect change can be hazardous and should not be done without full cognizance of the risks involved.

Merton's five modes of adaptation serve to underline the importance of knowing the social orientations of those with whom the health worker must deal and the potential assets and liabilities of such orientations for the practitioner's tasks. It is clear, if one agrees with the validity of such categories, that the innovative type of adaptation provides the obvious and easiest channel to change; that the retreatist is practically useless; that the rebellious is hazardous. There remain the ritualistic and conformist modes of adaptation. These are both difficult, but I would suggest not impossible, channels through which to work. Groups and individuals with a ritualistic adaptation may be fired to initiate change if they can be persuaded of new and more potent goals than the old ones which, for them, have lost primacy. Identification, in Kelman's sense, may fire ritualists to new goals and transform their ritualistic to innovative modes of adaptation. Similarly, groups and individuals with a conformist adaptation may be subject to persuasion precisely in those areas where they sense cognitive dissonance in their own culture, i.e., incongruities between values, incongruities between institutions, and incongruities between values and institutions. These areas of cognitive dissonance may be crucial leverage points for change in the conformist mode of adaptation.

I have reviewed Merton's modes of adaptation at some length because they provide the practitioner one kind of rough and ready way of analyzing in any society the groups or individuals who are most likely to want, and to be able, to effect change.

Goals, Roles and Induced Change

However, in every society, goals have different valences. It is analytically useless to say that in one society wealth is more prized than piety, or that in another society equity is more valued than justice. For the practitioner more trenchant formulations are needed. Again Merton(10) has a useful set of preliminary distinctions. In discussing social controls he distinguishes between prescribed and proscribed behavior, and adds two intermediate types that he calls preferred and permitted. In our society, for example, parental care of offspring is prescribed; murder and espionage are proscribed. Marriage with non-kin is preferred but permitted within bounds of cousinship. The point once made and labelled is obvious; this, however, does not mean that it is unimportant. Among the groups and individuals designated earlier as innovators there will be prescribed and proscribed goals. Should innovators disregard such goals they risk suffering social disapproval that would make such persons poor mediators of social change in the larger society. Therefore

it is in the realm of permissible and preferred behaviors and goals that the public health practitioner should look for ways of inducing change.

Spiro(11) gives this idea of kinds of social control a somewhat different and useful elaboration in respect to social roles. Like Merton he points out that goals are either sanctioned or prescribed. The prescribed goals are mandatory, the sanctioned ones approved. To use his example, the goal of becoming a physician is sanctioned, i.e., it is permitted and even approved, but to retain the status associated with the goal, the role required of the physician is prescribed or mandatory. That is, once a sanctioned goal is sought, the social role associated with it becomes mandatory. Why this should be so need not delay us here. The point of interest to us is the idea that roles can be as binding as goals; that social controls can arise from participating in institutions (which are simply congeries of roles) as much as from subscribing to or rejecting social goals. Again the implications for those who are charged with introducing change are clear. In any society groups or individuals whose roles prescribe behavior, and for whom deviation from their roles would entail social penalties or the abandonment of goals, are frequently limited agents of change. In our own society this is nicely illustrated in the relative flexibility of the politician compared to the civil servant. The politician's social role is far less subject to social control, is less prescribed, than that of the civil servant (although I am by no means sure that this is the case in every nation).

So far we have sought congruencies between the concepts and findings of investigators from different disciplines. The criterion for this search is that the concepts and congruities should provide analytic guides to public health workers who are concerned with induced socio-cultural change.

We have said that within the proceptive field of individuals, the strain for cognitive consistency may be, if properly used, a dynamism for change. But changes in behavior, attitudes, and values are often mediated by interpersonal relations and are of varying depth and permanence. Further, in any society the relationship between goals on the one hand, and institutional resources for achieving them on the other hand may be differently perceived by groups and by individuals. Varying modes of adaptation may result from the differences in perception. These modes of adaptation have varying potentialities for change. However, we must also recognize that goals have different valences. Similarly, the congeries of roles which constitute institutions vary in the extent to which they are prescribed.

Class and Socio-cultural Change

In developing this argument, two areas frequently included in any discussion of change have not been touched upon. The first is social class

and the other is culture-trait inventories. Reasons for giving them short shrift will now be discussed.

The relationship between social class and social change has been a subject of considerable research. Sorokin, for example, in a study of the class provenience of some 3,000 saints over 20 centuries reaches the conclusion "that as a rule the upper classes are the first innovators and importers of the new cultural values in a given society and its culture, and especially the first molders of its highest ideals and values. The lower classes ordinarily lag behind and follow or imitate when the values cease to live and function within the upper classes. In other words, the stream of innovations in a given society flows from the top to the bottom."(12) On the other hand, Barnett, working within the framework of cultural comparisons, says, "It is a transparent fact that the members of privileged classes have a vested interest in the system on which they thrive and that they resist changes in it which will deprive them of their advantages. Changes which threaten the social, political and economic prerogatives which they cherish are understandably not welcome by them. . . . When cultures meet, the majority of those who switch their allegiances are individuals with the least opportunity for full participation in the most valued activities in their own society."(13) Dr. Barnett would have difficulty reconciling this generalization with Dr. Sorokin's and neither, I believe, could make their generalizations stick. It is not that social class is irrelevant to social change, or to the problems facing a public health worker. Simmon's excellent article on the *Implications of Social Class for Public Health*(14) is ample testimony that it is important. But its importance lies not in class *per se* but in class as an index to cultural goals and institutional roles. For our purposes here social class is a dependent and not an independent variable. The public health worker responsible for analyzing a society must go beneath the surface of class groupings. As Karl Polanyi persuasively argues in *The Great Transformation,* "Mere class interests cannot offer . . . a satisfactory explanation for any long-run social process."(15)

Acculturation and Induced Change

Turning now to culture-trait inventories: — the older literature of acculturation in anthropology is replete with *obiter dicta* concerning those aspects of a society that are most subject to change. For example, Malinowski stated that "the vanguard of change is often found in 'works of leisure and supererogation."(16) By contrast it is often claimed that material culture and technology are the first aspect of culture to change in the contact situation and religion the last. But it is not in these external and relatively easily observable aspects of society that we find the most useful clues to where and how socio-cultural change occurs. Rather we must look to the socio-psychological factors that underlie these external manifestations.

And here, in the socio-psychological sub-strata of change recent research by anthropologists has possibly most to contribute to practitioners of induced change. Perhaps the best way to illustrate this area of research is to summarize very briefly Hallowell's classic study of acculturation.(17) Hallowell set up four levels of acculturation from the least to the most westernized of Ojibwa Indians. These levels are based on easily observable culture traits like clothing, language, food, etc. The least acculturated level cannot be studied directly but is known from seventeenth and eighteenth century travelers. The most acculturated are the Lac du Flambeau Ojibwa of Northern Wisconsin, practically all of whom speak English, 80 percent of whom have white blood, and whose children attend government schools. The signal finding is the "persistent core of psychological characteristics sufficient to identify the Ojibwa personality constellation, aboriginal in origin, that is clearly discernible. . . . All the evidence points to far more complicated psychological processes than those which have led to the acquisition of the culture traits which were used as empirical guides to the different levels of acculturation. Consequently, descriptive facts of this order are no direct index to facts pertaining to personality adjustment and personality organization."(18) In other words, it is not to culture traits but to psychological factors that one must look if the intimate and immediate dynamics of socio-cultural change concern us.

Hallowell's "principle of psychic persistence" has been confirmed by many other investigators working in different areas and with different techniques. "Anthony Wallace in a recent study of Iroquois acculturation, states a now perhaps widely held view that 'no cultural form can be successfully introduced, within the space of one generation' which 'requires behavior which is uncongenial to personality structure. Hallowell himself goes further in saying that it is 'hard to imagine' how basic sets of personality structure could be changed fundamentally in 'less than three generations.' Keesing, however, has suggested that if the crucial cultural surrogates (e.g., the mother or foster parents in an entirely outside setting) are sufficiently in the new tradition, the essentials of such a shift might be made in two generations — though this is quite exceptional."(19) Vogt(20) in a study of Navaho veterans suggests that value change occurs first through imitation and then internalization of white norms and that this process is facilitated (among other factors) by a lack of satisfying affective ties and by personal conflict and insecurity with respect to fellow Navaho. And that in general the acculturated Navaho is a second generation phenomenon — that such a man is the son of parents already deviant to Navaho culture.

We seem now to have come full circuit. We have been looking for congruence between various levels and fields of inquiry as to the nature of socio-cultural change. We have been particularly interested in those concepts that might assist practitioners of induced change. From anthro-

pological research in acculturation we find that Kelman's three processes of attitude change — compliance, identification, and internalization — may have diachronic (i.e., sequential) significance. One might suggest that compliance, identification, and internalization are equivalent to three stages of socio-cultural change and conceivably (although this is pure speculation) that they may entail as many generations. Also, in the course of such slow changes over generations, many groups and individuals will evince modes of adaptation that Merton suggests, namely: conformity, innovation, ritualism, retreatism, and rebellion.

But how are we to explain such slow changes when we know that human beings, as opposed to other social animals, have all the plasticity that learning as an adaptive mechanism provides? Bruner suggests the hypothesis of early learning to account for the principle of psychic persistence. He claims that what is transmitted early in a child's training is most resistant to change.(21) Although Bruner's hypothesis of early learning is persuasive and warrants wider investigation, and although it has the virtue of simplicity, I suspect that it is not adequate to the complexity of the situations facing agents of induced change. It may be useful therefore to review still another way of conceptualizing socio-cultural phenomena that may yield some explanation for the principle of psychic persistence, or, if you wish, for cultural stability.

Cultural Levels and Induced Change

Dr. Hall, long a collaborator of George Trager, has recently published a book called *The Silent Language.*(22) In it he follows the lead of distinguished predecessors, like Gregory Bateson, in treating culture as a communication system. We need not follow Hall's full argument but consider only the distinction he makes between formal, informal, and technical levels on which all dimensions of culture operate.

At the core of every cultural dimension whether it be temporality or territoriality, learning or play, subsistence or association, there is a formal level of culture. This is learned through precept and admonition. "Men never do such and such." "Don't say goed, say went." It is what we generally mean by "child rearing." People are fully aware of the formal aspects of their culture, but they do not question them. It is the "perfectly natural," "just common sense," the "human nature," "what everyone knows" kind of approach that is accepted without challenge. Patent violations of such formal aspects of culture arouse a flood of explicit emotions. Change in the formal aspects of culture come slowly.

Surrounding this core of the formal level of culture is the informal. The informal level of culture, like the formal one, is learned. However, it is not learned through precept and admonition, but rather through the imitation of a model and by observation. Informal levels of culture are customarily out-of-awareness and automatic. But if breached, anxiety

usually follows. Resistance to change at the informal level will be characterized by the defense mechanisms congenial to particular individuals or groups.

The third level of culture is the technical. It is transmitted explicitly from teacher to student, either orally or in writing, often accompanied by logical analysis and according to a coherent procedure. It is what we generally mean when we speak of education at the institutional level. The technical level of culture operates at the highest level of consciousness. Here change can be effected with far greater ease than in the formal and informal levels.

You will recall that I suggested earlier this evening, in connection with Festinger's work on cognitive dissonance, that it would be relatively easy to convince a community that an injection was a more effective way of curing yaws than a poultice because the change was in the *technical* realm. On the other hand, substituting a mosquito for grandfather's punitive ghost as the cause of fevers would be difficult because the cognitive dissonance would be too great. Now phrasing the issue in Hall's language, we can say it is because a formal aspect of culture is challenged; — one of those beliefs everyone shares, that is "just common sense."

Again, you will recall that I suggested using Kelman's concepts of compliance, identification, and internalization that one might through surveillance and/or attractiveness persuade a community to fill in mosquito breeding puddles, but that such behavior would have little chance of entering permanently into community practice. Now we may further argue that puddle-fillers were not models that people had learned, informally and through observation, to imitate when they were learning their culture; nor were they formally admonished to fill puddles. While technically to learn the relationship between puddles, mosquitoes, and malaria requires grounding in a whole system of thought not easily conveyed and one that might well test the credulity of people accustomed to trust their "common sense."

Dr. Hall's thesis, needless to say, is far more complex than this hasty and partial reference indicates. But if one considers only his three levels of culture and if one considers that these three levels tend to reinforce each other over time through the operation of the strain for consistency, then it becomes much clearer why socio-cultural change is a slow process, why it is often attended by personal dislocation, and why the public health worker whose professional orientation is "putatively" purely technical is frequently bewildered by the resistance he meets in attempting to introduce changes.

Before leaving Dr. Hall's thesis, I should like to quote one of his summary sentences. "Change is a complex circular process. It proceeds from formal to informal to technical to new formal."(23) In other words,

Dr. Hall seems to see socio-cultural change in terms of a feedback system in which cause and effect are not seen as linear but circular. And as Bateson says, drawing on cybernetic theories, "Such circular causal systems must in the nature of the case either seek a steady state or undergo progressive exponential change; and this change will be limited either by the energy resources of the system, or by some external restraint, or by a breakdown of the system as such."(24) We all know from history that societies can seek a steady state, may be limited by their energy resources, have been restrained by external forces, or have broken down.

We may assume that the public health worker aims to limit socio-cultural change to the resources of a society without producing a breakdown of the system and without making it endlessly dependent on external resources. If this be a fair assumption, then we may turn to the final issue of this paper.

The field worker is on the firing line of any public health program. He bears the burden of trying to induce change. His skills, insights and sensitivity are rightly considered crucial to the successful implementation of any plan. It is therefore customary on occasions such as these to issue them encouragement or warnings, particularly if the program is an intercultural one. It is customary to remind them of "culture shock"; or to urge them to recognize and use their privileged position as strangers; or to insist that their work rest on surveys (which are usually far too superficially descriptive to be useful on any dynamic level of analysis); or to warn them that they too bring with them a series of valuational prejudgments as well as technical skills, or to provide them with illustrations of difficulties ingeniously analyzed and surmounted elsewhere (but which are unfortunately usually so specific that they hardly serve as guides to other situations).

None of these is my intention. There is no need to focus again on the long-suffering, much-harangued field worker or to give him more gratuitous advice than he already has had. Rather my intention is to focus on the planner of programs in public health.

Much that has been said bears directly on what should be properly planned, what are the legitimate expectations from such planning and what may be realistically expected of the field technician.

Planning for Induced Change

First of all, planning is rationalizing. It is not only theoretically at the technical level of culture; it is also often an attempt to impose a technical approach (in Hall's sense of the word) on the formal and informal aspects of culture. However, if we see change as a complex circular process in which activity at the technical level feeds back into the formal

and itself becomes a formal aspect of culture, then planning itself must beware of becoming as unquestioned, as "perfectly natural," as any moral precept in the formal aspect of culture. Its rational and secular quality thereby risks being vitiated. I suspect this is particularly true in societies where planning is prescribed rather than preferred.

Ideally, the objective of any plan should be unambiguous, internally consistent, and quantifiable. But as Dr. Eckstein has so well demonstrated in his interesting study of the English Health Service, this is rarely possible.(25) It therefore behooves planners, while striving to keep their objectives clearly in mind and to cope technically with their problems, to remember that there is usually considerable "slippage" for precisely the reasons here discussed. As Eckstein so well puts it, "We need only realize that the engineering reason [for which, in Hall's terminology, we may read technical reason] is rigidly circumscribed and liable to falter altogether . . . and that the fully rational direction of social activities is possible only under one condition: if the norms we impose on conduct, whether relating to ends or means, are precisely those highly delimiting norms that make rational action possible."(26)

Competent planning must in the first place recognize what level or levels of culture are likely to be most immediately affected. As we have said earlier, and as Eckstein clearly indicates, planning is a technical process. If it deals with a technical level of culture, planning is relatively easy and may be reasonably precise. For example, there are difficulties, but no insurmountable ones, in campaigns of mass inoculation or vaccination where one competes only with a different pharmacopia in the indigenous society. As Festinger might say it, the dynamics of cognitive dissonance can be constructively employed. Under such circumstances, given sufficient resources and assistance, disease may be eradicated if specifics for the diseases are known. In such situations, it is relatively simple to achieve compliance through surveillance, to use Kelman's language, and eradicate smallpox or yaws. Such enterprises are the traditionally cited success stories of public health. The formal two-year, five-year, or ten-year time spans for such undertakings may still be within the compass of rational, engineering, or technical programs (depending on the language that is congenial).

If, however, the program entails alterations in the informal aspects of culture, that is, in the models people learn through imitation and that are largely out-of-awareness, if, for example, alterations deal with habitual cultural orientations in time and space the task becomes of necessity a long and subtle one. Change at this level must be reckoned not in two-, five-, or ten-year programs, but on a generational basis. The same holds true for alterations that deal with the formal level of culture. Persistence and persuasion will be necessary. Here Kelman's process of identification must occur in the first generation before the second generation may, at

best, actually internalize the alterations. But, even this suggestion may be too optimistic. If we use Hall's concept of informal and formal levels of culture; if we combine this suggestion with the principle of psychic persistence in which a minimum of two generations is essential to effect changes in personality structure; and if we add Kelman's suggestions of identification and internalization in a diachronic sense, and assume that the first generation adopts a new response as a value but isolates it from other values, whereas the second generation integrates the new response with other values — if we make all these assumptions, the model is still far too simple. For it implies that the interpersonal relations essential for such transformations of informal and formal aspects of culture to the technical level have operated with maximal effectiveness. In any population aggregate, nothing is more improbable. Rather, there will be the types of "wastage" that Merton has categorized in his modes of adaptation as ritualism, retreatism, and rebellion.

Planners of public health programs who ignore such psycho-social and socio-cultural processes are 1) misleading themselves, their organization, or their governments; 2) are wasting their material resources; 3) are disheartening their field technicians, and 4) may be unwittingly encouraging unconstructive modes of adaptation. Planners and trainers of public health workers, alert to such considerations, would never insert in a manual for rural community health workers, (I quote) "To more or less complete a program in a community will probably take at least two years, especially if adequate educational results are to be obtained along with such physical improvement as privies."(27)

This sanguine suggestion flies in the face of both experience and theory. It illustrates better than anything said in academic language the fallacies of assuming that socio-cultural change can be usefully planned on *only* the technical level and of assuming that psycho-social learning is *solely* a matter of rational indoctrination. It ignores that goals must be identified with; that the innovative elements of society must have opportunities to develop institutions that support such goals; that the learning of much of cultural behavior requires identification through models and precepts before it can be internalized; and that the integration of new goals and behaviors with the rest of experience requires shifts in the consciously held formal levels of culture, as well as in the informal levels of culture that are out-of-awareness.

Needless to say, I have not spoken tonight of socio-cultural change achieved by force and the liquidation of resistive elements of a population. Such changes I have assumed are not techniques appropriate to a public health program in a democratic society.

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OPERATIONAL RESEARCH IN HEALTH EDUCATION

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Keynote address made at the annual conference of the Society of Public Health Educators, St. Louis, Missouri, October 25, 1958.

I would like to start with some of the possible treatments of "operational research" which I am *not* attempting here. (1) I am not *selling* "operational research" as the answer for: (a) the upgrading of the profession of health educators; (b) the filling of all gaps in the professional knowledge needed by contemporary health educators; or (c) the major tool for transforming health education programs toward greater effectiveness and efficiency. I sincerely believe that "operational research" can contribute to all of these desirable outcomes. But I also believe that critical evaluation *by* professional leaders of what is called variously "action," "operational," and "applied" research is better calculated at the present time to advance these contributions than *selling* to professional leaders. (2) I am not trying to demonstrate verbally that operational research, stimulated, led, and carried through by social practitioners in the interest of more adequate professional policies and programs, is possible. This may well have been useful when John Collier and other pioneers began to urge the feasibility and desirability of "operational research" a quarter of a century ago, but we now have numerous examples of operational research to point to in many professions. Its possibility has been demonstrated in fact. What is needed now is critical appraisal. (3) I am not trying to explicate the many technical problems of problem selection and delimitation, experimental design, construction and adaptation of instruments for data collection, sampling, etc., which dog "operational researchers," as they also dog "basic researchers," though with added complications for the former. Dealing with such technical problems is important but more appropriate to an extended seminar than to a brief professional conference.

A Context for Critical Appraisal

What, therefore, I propose to do is to take the stance of the sympathetic but critical appraiser of "operational research" in the professional activities of health educators. The critic needs to place what he is appraising in a context which permits him to relate it to other alternatives and to a test in terms of its wider consequences, its feasibility, and its desirability.

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In what context should we place "operational research" in appraising it, particularly from the standpoint of professional health educators?

We are now living in the midst of a dangerous and widening gap between "common sense" and "science" as arbiters of choices and decisions in human affairs. (I prefer this polarity to the all too familiar dichotomy of "theory" and "practice." For "common sense" has its theoretical aspects, often sweeping ones, though frequently unacknowledged and unsystematized, as well as its practices and techniques. And "science" has its practices and techniques, implicit and explicit, as well as its "theories.") Do health educators, along with associated lay boards, councils, and committees, decide what programs to undertake, what program goals to emphasize or de-emphasize, what methods and organization to employ on the basis of "common sense" or on the basis of "science"? Is "common sense" (an amalgam of traditions, lore, wisdom, principles abstracted from practical experience, the advices and preferences of reigning elites) more dependable, reliable, and valid as a basis for policy and program construction and reconstruction than knowledge drawn from basic research in the behavioral sciences or knowledge derived from the application of scientific methodologies to the study of policy problems in the setting of practice?

The gap continually widens between the two methods as scientific research is pursued in physics, chemistry, and biology, and as related engineering or applied science disciplines use scientific methods to translate these findings into technologies in the practices of health, industrial production, war and defense. In human affairs, a vocal and influential part of our culture counsels dependence upon *common sense,* and its rationalizations in traditional humanities and religions, and opposes the extension of science into the solution of human problems. Other parts of our culture promote the extension of *science,* basic and applied, in the management of human affairs. So the split becomes a social and cultural, as well as an intellectual, split.

And the split extends also into the persons who must negotiate this divided culture in their life and work. This split is nowhere more poignant than in professionals in the fields of social practice. Health educators are taught scientific approaches to health and disease, diagnosis, treatment and prevention. Their task is to communicate this knowledge to people in a way to affect their conduct with respect to the maintenance and restoration of health and the prevention of disease. But how are their policies and programs for communicating with people, influencing their learning and conduct, organizing them both to learn and to act in health matters, determined? Partly no doubt on the basis of scientific research into the behavior of people as persons, groups, and communities or into the processes of their learning, communication, and motivation. But much more fully, I venture to say, on the basis of "common sense" notions of

human conduct, human learning, of distribution of power and prestige in situations, of conventional assumptions with respect to what can go and what can't go in a group, an organization, or a community. And counsels to practice from the two bases don't always jibe. The result is conflict within the person of the professional.

How does "operational research" fit into this gap? It is an effort to bridge the gap – to bring the methods and approaches of the behavioral scientist into the processes by which program priorities are determined, by which programs are defined, by which program effects are evaluated. It goes one step further than drawing upon the "products" of behavioral research and translating these into program meanings and implications. It involves the practitioner and his associates in using the methodologies of the behavioral scientist in extending and validating their operating knowledges and techniques, in assessing and evaluating program success and failure, and in correcting and reconstructing relevant "common sense," both in themselves and in their associates.

Criticisms of "Operational Research" From Fore and Aft

It is not surprising that efforts in "operational research" have met with criticisms from both "anti-scientific" and "scientific" sides of the current split. An extreme case of the former is Whittaker Chambers who in *Witness* equates scientific method in human affairs with communism, his word for the devil. In making our judgments of what should be done he believes we must rely on our traditions, upon God, not upon human intelligence or experimentation. Other anti-scientists urge, more temperately, that human values are lost in any scientific or engineering approach to human problems, that science is amoral if not immoral, that wisdom comes from sources other than scientific study – in my terms, from "common sense."

Raymond Cattell,(1) psychologist, speaks for many on the "scientific" side:

> "I should . . . point out . . . a bastard philosophy, very prevelant in some social sciences under the name of "action research," which mingles "revealed" religious ethics, i.e., non-scientific, ready-made ethics, with the experimental principles of social science and attempts to call the offspring "scientific" and to claim for it the prestige and character of science.
>
> "Every recommendation in "action research" argues first from a chain of truly scientific mechanisms – if A is done, then B and ultimately Z will follow – and secondly from a value system – because Z is good, A is good. It is my conviction that confusion of thought and damage to scientific prestige will result if we permit these callow enthusiasts to apply such labels as "research" or "science" to their recommendations unless they first demonstrate that their statement, "Z is good," can also be made from a scientific basis. This has not been

done; indeed, naive "high school" moral values have been combined with "college" science without the glimmer of a realization that this great new possibility of a totally scientific ethic exists or that action research is 50 percent unscientific."

What can we learn from these fore and aft criticisms of "operational research," pertinent to our present purpose of appraisal? Allowing for the rhetoric, the slogans, posturings and polemics, which mark the language of partisans in any emotionalized split in a society or culture, one clear issue seems to emerge. This is the unavoidable confrontation with value judgments, judgments of what should and should not be, in the program and policy decisions which practitioners must make one way or another. Setting the goals of a program, allotting priorities among specific goals, require value judgments and, in our divided communities, these mean unavoidably some choice with respect to value-issues and value-conflicts.

How competent are the methods of science in making such choices? How far can research methods solve problems which involve decision upon issues of value? Is science as a human pursuit devoid of values? Must the practitioner, as he chooses and decides, operate on extra-scientific values? If so, how are these validated? Can the values of science be integrated with such extra-scientific values inherent in the "common sense" of the situation confronted? If they clash, which must yield? It is in relation to questions such as these that we must undertake a critical appraisal, must assess the virtues and limitations of "operational research" in the processes of professional functioning and development.

The Common Meeting Ground of "Common Sense" and "Science"

The common meeting ground for "common sense" and "science" is in processes of problem-solving. Most theorists appeal to the similar form of problem-solving in situations of practical judgment and of basic research in urging the extension of operational research in professional and practical affairs.

Stephen M. Corey,(2) for example, identifies five stages common to both processes and problem-solving: defining the problem; hypothesizing actions and predicting effects; designing a way of testing the hypotheses; collecting evidence; generalizing the results. We may agree, I believe. that processes of practical judgment and scientific research involve these subprocesses. And, in a fundamental sense, the goal of operational research is to bring the methods and disciplines of the scientist into the informing and rendering more valid of processes of "common sense" judgment in any or all of its phases.

But in what ways do the values of the basic researcher and of the social practitioner enter differently into their processes of problem-solving? In the answer to this question, we may find some clearer view of the difficulties of extending operational research in the life and work of professional practitioners. The following analysis suggests both similarities and differences in the value orientations of the two social roles.

The Role of Behavioral Scientist and the Role of Social Practitioner (Health Educator)

Behavioral Scientist	Health Educator
1. Both make choices in their work and so must be guided by some system of values as well as by professional knowledge and skills.	
2. On top of his hierarchy of values is valid knowledge-building about the behavior of people — values of *improving* the functioning and intelligence of people he studies is secondary.	2. On top of his hierarchy of values is *improving* the functioning and intelligence of people with whom he works — extending basic knowledge of their behavior is instrumental to this aim.
3. People with whom he works are subjects who serve his purposes of extending generalizable and valid knowledge — he is not interested in particular cases except as instances to confirm or invalidate generalizations.	3. People with whom he works professionally are clients — he is concerned centrally with particularized cases and situations in order to apply knowledge in improving these.
4. Both are influenced in their choices and work by persons and associations significant to them in evaluating, rewarding, punishing their efforts and contributions.	
5. The scientist is particularly influenced by persons and associations in his field of scientific specialization — he is shielded from direct influences from men of action and social power as determinants of his choices.	5. Professional associations are one reference group for the health educator — but he and the profession by the nature of their work are more directly influenced by social need, by men of action, and men and groups in power positions, as coaches of their decisions.
6. Time in the form of pressing decisions does not influence his judgments and choices so directly as it does those of practitioners — he can reserve judgment, waiting for the accumulating weight of evidence — a longer time perspective operates in his judgments of what needs to be done now and later.	6. Time presses the practitioner to decide and act — judgments can not wait — he must judge in order to meet deadlines, whether the evidential basis for judgment is "complete" or not.

What Then Are the Key Problems in Extending Operational Research in Health Education Settings?

a) *Effective operational research will require some integration, preferably conscious and deliberate, of the role of behavioral scientist with the role of social practitioner in the persons of the health educator and of those with whom he works.* This is true from the other direction for the behavioral scientist who cooperates and consults in such researches. This means not alone an integration of skills and techniques but of values and role-prescriptions as well. There are certain to be tensions and compromises in such processes of integration. These should be accepted

without guilt or denial. The resulting problems chosen and research designs effected will not be equivalent to either basic research designs or "common sense" patterns, unless the health educator tries to deny his social practitioner role, "makes like" a basic researcher, and tries surreptitiously to slip "basic" research designs into his work, unknown to his lay associates. This is not desirable ethically nor, in the long run, is it prudent.

We can predict from this role analysis some of the characteristics of research problems and designs consonant with practitioners' values. They will serve educational purposes for those who take part as well as knowledge-building purposes. They will involve greater degrees of cooperation in planning and decision on the part of "clients," become for the time "subjects" as well, for study by self and others. Problems for study will be chosen for the promise of practical pay-off in the results of study. Tensions in practice will provide a way of locating problems to be studied (the practitioner's way) along with gaps in existing professional knowledge (the scientist's way). Both criteria should be used simultaneously. Variables selected for study will be chosen in terms of their accessibility to manipulation by practitioners and clients and of their strategic importance as well as their probable relationship, theoretically, to the more adequate explanation of phenomena studied. There will be greater interest in the processes of interaction among many variables in particular concrete cases or situations than in the isolation of single variables for study in a wide variety of situations. Values and goals will be studied along with non-normative facts and factors. Yet the two should be distinguished — the operational researcher must become in some measure philosopher as well as scientist-practitioner.

This is as it should be. Operational research is not better or worse than basic research; it is different, because it serves somewhat different purposes. Yet both can be "scientific."

b) *Inquiry Rather Than Justification.* But the results of operational research will not be scientific if the value system of the scientist as well as his methods and techniques are not taken over into the integration. For science has inherent values, an inherent ethic, however much some scientists and anti-scientists may deny it. It may be summed up as the spirit of inquiry. This clashes with the spirit of justification which pervades much "common sense" evaluation and diagnosis. How can the spirit of justification be validly replaced by, or strongly tinctured with, the spirit of inquiry? Many health educators work for agencies and organizations, public and private, which must justify their existence, their way of working, to the persons who are sources of funds and of moral support and who apply over-simplified and emotionalized criteria in judging success or failure. What if evaluation research applied to our program gives evidence that we are not living up to our public relations and fund-raising appeals? What if admitting the need for research on what we are doing

is taken as evidence that our agency doesn't already know what it is doing? Studies undertaken in this atmosphere are likely to be distorted in the image of justification, and the spirit of inquiry will be a casualty in the process. It is better not to go through the forms of inquiry, if the conditions necessary to support valid inquiry are not in some measure present.

This is no easy problem. The answer seems to lie in two directions. First, choose problems where some readiness to inquire and to abide by the results of inquiry already exists. This may mean tackling less crucial problems first. Second, use participation of key people in the inquiry to widen their inquiry span, to extend the areas in which they are willing to inquire rather than to justify and reenforce what they already "know" on the grounds of "common sense."

c) This latter discussion suggests what has long been recognized — that *operational research is a methodology for training and change as well as for gaining new knowledge.* The latter goal must not be lost sight of, lest the spirit of inquiry be lost and "scientistic" manipulation of attitudes and values take its place. But the former goal needs to be kept in mind too in selecting problems and designing studies. Specific learnings about communities, programming, prevention, epidemiology, etc. will result from participation in particular operational researches. These learnings are important, but they are not the basic learnings. The basic learnings have already been suggested — skill, confidence, and commitment to the use of the values, methods, and resources of the behavioral sciences, along with those of "common sense," in analyzing and solving practical problems that confront us now and in the future. This means helping to heal the cultural and personal split between "science" and "common sense" in human affairs.

d) *Using Scientific Expertise Wisely.* Expertise from the behavioral sciences will often be needed by practitioners in meeting the technical problems which operational research present. The main goal is to develop a cooperative, non-defensive relationship between expert and practitioner. Health educators must work to overcome whatever inferiority feelings they may have as "mere" practitioners in the presence of "scientific" expertise. They must accept and discount the hesitations of many behavioral scientists who naturally fear losing caste with their pure peers by risking the impurity of contact with practice and action. Understanding of the differences in roles, mutual respect for the different values and value-hierarchies involved in these roles, agreement on plans which invalidate neither set of values — these are the substantive goals of research expert-practitioner relationships that must be sought and built.

e) *Strategies of Installing Operational Research.* Some general points of strategy have been suggested in the previous discussion. These may be pulled out in summary fashion at this point.

1) Health educators are in the first instance social practitioners. If they are to do whole-hearted "operational research," they must recog-

nize the differences of value orientation and emphasis in the roles of "social practitioner" and of "scientific researcher" and must work to integrate these two roles without denying either. Wisdom in effective operational research, as elsewhere, begins at home.

2) People accustomed to functioning as *clients* vis-a-vis health educators must in operational researches function as *subjects* of study, furnishing data, having their behavior analyzed and interpreted. There must be sufficient cooperation by clients in deciding to do research on a policy problem and in determining what problem to study to insure that they are willing to become *subjects* as well as *clients* and to accept the pains and tribulations of the former estate. This means that erstwhile *clients* must understand and accept the differences between the roles and relationships of *clients* and *subjects* and accept the latter role in the interest of eventual program and policy improvement. Reeducation along this line should in some degree precede the installation of an operational research project as it most certainly will follow if the results of the research study are taken seriously in subsequent policy and program decisions.

3) Initial problems should be selected in a way to meet the criteria of both practical importance and "researchability." This means that an initial problem should be one which those concerned, including the power persons and groups related to the program, can approach in a spirit of inquiry, ready to change ideas and practices if the study indicates the need for change, rather than a spirit of justification, in which results are selected, twisted or rejected in order to maintain and justify established ideas and practices.

4) The help of research expertise will often be needed by practitioners in dealing with the technical problems they are certain to encounter in installing researches. A relationship of equal status cooperation needs to be developed if such consultation and collaboration are to work well. This reemphasizes point 1 above which applies not alone to practitioners but to behavioral scientists as well, though in reverse direction.

The Special Opportunity for Health Educators in Operational Research

1) Health educators work in a field with an overall value — improved public health — that is less riddled by controversy in current American culture than are the fields of welfare, economic policy, international relations, etc. True enough, health policy verges at times on all of these — as controversies about socialized medicine, fluoridation, etc. indicate. But a core value, common to the American public, is the central concern of their profession. This is a position of strength in efforts to bridge the gap between "science" and "common sense" as determinants of public policy and program.

2) The health education profession is probably less encrusted with its own "common sense" traditions than any other health profession.

It is, therefore, probably freerer than any other to adapt its traditions to new currents of thought from the behavioral sciences.

3) It is oriented to "education," and operational research in its installation requires an educational approach, just as its processes are inherently educational in effect for those taking part, if validly carried through.

4) Its relative lack of prestige among health professions may be a disvalue as well as a value in getting things started. But, in the long run, it may have more value, if the profession itself does not translate lack of prestige into an unjustified feeling of inferiority. For changes in the practices of less prestigeful professions are sometimes less ominous and so less subject to irrational resistance from other collaborating professions than are changes in the more prestigeful collaborators.

I, therefore, commend your past efforts and look forward to your future efforts in operational research designed to advance the improvement of your profession. You can by these efforts, I feel sure, contribute not only to putting your own professional activities on a firmer basis of tested knowledge and value but also contribute to the will of other professions to heal, in their own spheres of professional activity, the dangerous split between "science" and "common sense" in our divided culture.

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